

Interactive comment on “Using hydrologic landscape classification and climatic time series to assess hydrologic vulnerability of the Western U.S. to climate” by Chas E. Jones et al.

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Thank you for offering us the opportunity to respond to your comments and feedback on our manuscript titled “Using hydrologic landscape classification and climatic time series to assess hydrologic vulnerability of the Western U.S. to climate”. The manuscript was reviewed by one reviewer (RC2) that recommended that the paper be “revised and resubmitted”, while you (RC1) suggested changing the manuscript into a technical note. We believe that we can effectively address the specific concerns in an additional revision that will improve the manuscript and make it more worthy of publication in HESS. Overall, we found the reviewer feedback to be insightful and are certain that it

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will ultimately benefit the manuscript. Below are our responses to two reviewer comments and one short comment Please note that the specified line numbers reference the original manuscript submitted to HESS.

RC1 Comment 1) The manuscript, using existing indices and geospatial datasets, proposes a framework/rule-based decision making on the vulnerability of the Western U.S to future climate change. The manuscript is interesting and encompasses significant data management and GIS work. My general comments: Reading the manuscript, I have a feeling that HESS is not really the right journal for this work. Although interesting work, the manuscript seems to be a report/technical memorandum that is turned into a scientific manuscript. I would suggest this work may be better presented in other engineering or water management journals. This is just my recommendation on better presenting the work in its context to the right audience. Following that, it is rather difficult to provide a scientific feedback to this work. My feedback remains mostly on the clarification of presentation.

Author Response 1) We are pleased that the reviewer found the manuscript interesting, although we respectfully disagree with their suggestion that this research would be more appropriate for an engineering or water management journal. There were few concrete reasons provided by RC1 to justify such a conclusion, especially given that, by their own admission, they provided little scientific feedback other than on presentation. This was in direct contrast to RC2, who provided specific critical feedback that was helpful in improving the manuscript.

More specifically, according to HESS's website: "HESS encourages and supports fundamental and applied research that advances the understanding of hydrological systems, their role in providing water for ecosystems and society, and the role of the water cycle in the functioning of the Earth system." We feel that this manuscript is very well suited to HESS as our research uses an integrated approach to examine how hydrologic function drives the vulnerability of certain geographies and socio-economic industries to climatic impacts. We analyzed how basic hydrologic function might change

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across the Western U.S. under future climate predictions. We applied a hydrologic vulnerability index across the entire landscape and then examined how the vulnerability indices could be applicable to economically important industries but could also be informative about how the impacts of climate change may impact ecosystems and society at landscape scales.

Thus, we believe that HESS has the ideal audience for this research. This work is much less suited for a water management or engineering journal, as we do not provide research that is very relevant to water management studies or water resource engineering. We are also not presenting research about decision management tools or water management.

RC1 Comment 2) The use of English language is very good. The flow of the manuscript is smooth.

Author Response 2) Thank you.

RC1 Comment 3) I am not sure if I really understand the linkage between the hydrological landscape classification and the current manuscript. As the authors mentioned in the introduction, the landscape classification is usually at finer resolution than catchment scale. What the author are doing, is more of clustering or zoning of possible system response to climate change (similar to hydrological modeling approach but with less hydrology as only indices are used). The AU are just a unit where the data is compiled at and this is not really linked to the sub-catchment variability intended landscape classification at catchment level.

Author Response 3) We respectfully disagree with the reviewer's interpretation of the Hydrologic Landscape approach with respect to two major issues. First, Hydrologic Landscapes have never been applied at scales finer than at what we define as the catchment scale. In fact, Tom Winter, who originally developed the Hydrologic Landscapes concept in 2001, distinguished between six types of generalized Hydrologic Landscapes, which were conceptualized as large landscape units. When these were

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originally mapped by Wolock et al. in 2004, it resulted in 20 non-contiguous regions over the entire US, which were much larger than the catchment scale. The Hydrologic Landscape approach applied in the current application are at a much finer resolution than the Wolock et al. maps, yet are still a comparative analysis between catchments, and not an analysis within catchments. Secondly, while the original Wolock et al. Hydrologic Landscape analysis was in fact a clustering approach, the current analysis, which derived from the approach championed by Wigington et al. 2013, is not. In fact, the Wigington et al. approach specifically chose not to use a clustering approach, and instead used a conceptual approach that selected a priori important factors known to contribute to hydrologic flow. Furthermore, more recent studies using the hydrologic landscape classification approach have been applied at a subcatchment scale (Leibowitz et al. 2014, Patil et al. 2014, Leibowitz et al. 2016, Todd et al. 2017), using a similar approach as used in this analysis. In fact, we use the same subcatchments used in the Leibowitz et al. 2013 study and applied their method to delineate the subcatchments for our study's expanded 6 state study region. We could add further clarification of this process, however, in favor of brevity, we prefer citing the methods used in previous studies, which we simply applied to our expanded study area.

RC1 Comment 4) It seems the authors have a decision tree in mind that they use for classification using the input data. I would suggest the author to provide a schematic of their decision or algorithm that provide readers with better understanding of the method. Similarly, there is no visualization of the shapefile/regions used to create the vulnerability map.

Author Response 4) We can explore the idea of developing a graphical depiction of the hydrologic landscape classification approach, although the classification specifics are described in the methods: FMI (L187-188); Seasonality (L201-203); Subsurface Permeability (L211); Terrain (L217-219); and Surface Permeability (L225-227). We can also further clarify that the vulnerability maps depict the ~24,000 AUs that were classified for each Vulnerability parameter, since it wouldn't be very helpful to create a

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map that shows the ~24,000 AUs.

RC1 Comment 5) I would say the context of vulnerability is missing here. What is it used for? What is the intended motivation behind this vulnerability assessment?

Author Response 5) The vulnerability index can be used to identify areas that are most likely to experience altered hydrologic conditions in the future relative to the 1900-2010 period. The intended motivation behind this vulnerability assessment is to help stakeholders determine how likely they are to experience changes to one or more hydrologic parameters that are important for them or a particular industry / application.

RC1 Comment 6) (Section 3: Results): The result section is presented very quickly in (few) paragraph(s).

Author Response 6) The text portion of our results section occupies 8 paragraphs, which equates to 3 out of 14 pages or 1800 out of 8091 words (22% of the text of the primary manuscript). This does not include tables or figures that will ultimately be placed within the section. We believe this provides a section that is thorough and sufficiently summarizes the results. This seems relatively reasonable and we wouldn't want to expand it further without feedback on specific deficiencies or direction from the editor.

RC1 Comment 7) Section 4: Discussion): The discussion is kind of back to front. It is rather wordy. I would say it can be significantly shorter and focused on the interpretation of the results given the aim of this study.

Author Response 7) This comment would be challenging to address as it lacks specificity. We can certainly examine the content of the discussion and look for opportunities to be more brief, while maintaining clarity. We will also need to balance this comment with relevant comments by the other reviewer. That reviewer specifically suggested adding a subsection to the discussion to add clarity, which was helpful and should ultimately improve the paper.

RC1 Comment 8) (Section 5: Conclusions): Conclusion session is very vague. I would suggest the authors to come up with few bullet points Conclusions which readers can have as take home message. Also, the discussion, my pervious comment, can evolve along the line of the conclusion (I mean bullet point conclusions can help discussion significantly).

Author Response 8) It seems likely that the conclusions section will change as the manuscript evolves. We can highlight our take home messages. We can also examine whether there is an opportunity to utilize bullet point summaries in the discussion.

RC1 Comment 9) My overall suggestion is to change the manuscript into technical note. I would strongly suggest shortening of the manuscript and remove wordy sections (for example, in discussion). Explain the decision tree visually and elaborate that in methology section. Present the forcing and geospatial data in the decision tree and also visually. I believe major revision is inevitable.

Author Response 9) We can attempt to shorten the discussion section, which at present is 1615 out of 8091 words (20% of the manuscript), although we note that this is slightly shorter than our results section which the Reviewer thought was much too short. We would also prefer to shorten the paper overall (which aligns with this reviewer's feedback) and will look for opportunities to do so. We can also look for opportunities to summarize the methods visually with a flow chart (as a decision tree wouldn't quite be a proper fit to our analyses).

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2019-638>, 2020.

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